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A re-consideration of rates of ‘social mobility’ in Britain: or why research impact is not always a good thing

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Abstract

This paper re-considers some of the evidence for low and declining social mobility in Britain, showing that one study based on a re-analysis of cohort figures appears to have had an impact on policy-makers out of all proportion to its scale and rigour. The study claimed to show that the income of parents and children were more closely related for sons born in 1970 than in 1958, and that therefore social mobility was declining. It also claimed to show that the incomes of fathers and sons were more closely related in Britain than in countries such as Norway. However, a reconsideration of the same results in this paper leads to very different conclusions. This example is considered in detail here to illustrate the point that it is not always a good thing for research to have influence. The most important and ethical challenge facing social research in education is to improve its quality rather than its impact.

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The importance of research impact

Publicly-funded research in areas of public policy, such as education, ought to be both high quality and relevant to potential users such as policy-makers or practitioners. Since at least the mid-1990s, education research in the UK has been criticised for its too general lack of relevance and quality (Gorard et al. 2004), and many initiatives have been launched that include better linkage between research and policy/practice as their aim (such as the establishment of research centres, evidence portals, systematic reviews, and the Teaching and Learning Research Programme). These initiatives face a difficult task because, of course, researchers and the research community cannot completely control the extent to which their findings are read or acted upon. The frustration expressed by Halpern (2005), on encountering the selective and uncritical way in which policy-makers treat evidence, will be familiar to many researchers and the study discussed in detail in this paper provides another example of such difficulties. One lever that is more fully under the control of the researcher is the quality of the research and its reporting. An over-riding problem with initiatives to improve research impact is that whereas quality research without impact still has the potential to be useful, research impact without research quality is worse than useless. In this paper, I illustrate this point through consideration of a case study of recent research with, apparently, very high impact (at least in the sense of claims of influence reported by powerful commentators).

A study of intergenerational mobility

In a recent speech while she was Secretary of State for Education and Skills, Kelly (2006) claimed that ‘Social mobility in the UK has declined, and remains low by international standards’ (p.5). This is a direct and attributed reference to the study discussed in detail in this paper. Her successor, Alan Johnson, claimed to put social mobility at the heart of his personal agenda

(<http://news.independent.co.uk/uk/politics/article1578723.ece>). As Chancellor of the Exchequer, Gordon Brown repeated the same claim about social mobility in Britain, and suggested that overcoming this issue will be a key to his political endeavours (<http://society.guardian.co.uk/socialexclusion/story/0,,1256901,00.html>). Given that, at time of writing, Brown is now Prime Minister of the UK, this issue takes on an added importance. These politicians are all basing their shocking claim on, and making direct reference to, the findings of a highly influential study conducted in England by a team funded by the DCFS and the Sutton Trust (Blanden, Gregg and Machin 2005).

The explanation for this decline in social mobility, according to Sir Peter Lampl, chairman of the Sutton Trust educational charity which sponsored the research, lies in the declining standards of education available to the less well-off. In the past, grammar schools offered such people an opportunity for self-improvement. 'The comprehensive system,' he says, 'was brought in to try to improve social mobility, but the opposite has happened' (<http://www.suttontrust.com/press068.asp>). The research finding has influenced a UNICEF report, is explicitly channelled by the Institute for Public Policy Research, the Social Market Foundation and other think tanks, and is now accepted as fact by many social and media commentators. As a piece of research it could hardly have had more widespread acceptance among the influential and their influencers.

The Blanden et al. (2005) study purports to show that intergenerational social mobility in Britain is low by international standards, and getting lower over time. Thus, despite considerable state expenditure on education and welfare, the children of poor families tend to be poor and the children of rich families tend to be rich. In this account, Britain is very far from being a meritocracy, even though comparator countries such as Norway, Sweden and Canada are reasonably meritocratic. The researchers attribute the change over time, and hence the poor international standing of Britain, to a variety of factors. Most notably, they suggest that grammar schools were promoting social mobility in the 1950s and 1960s, providing an escape route from poverty for talented working-class children. Grammar schools have now been largely abolished in Britain. The authors also argue that more recently, in the 1980s and 1990s, the increase in participation in higher education (HE) has

disproportionately benefited children from middle-class families. ‘According to the LSE, the extra places made available by the expansion of universities in the Eighties and Nineties have largely been filled by students from better-off backgrounds’ (<http://www.suttontrust.com/press068.asp>).

The research appears on the website for the Centre for the Economics of Education (CEE), of which one of the authors (Machin) is the Director. CEE was set up in March 2000, funded by the taxpayer through the Department for Children, Families and Schools (DCFS) in England. It is based at the London School of Economics, the Institute for Fiscal Studies, and the London Institute of Education. The website claims that ‘All three partners have a proven and distinguished track record in the economics of education and training, and a great breadth of knowledge on policy relevant issues relating to education, training and the labour market’ (<http://cee.lse.ac.uk/>). Given the substantial funding that the CEE has received from the taxpayer, and charitable benefaction, and the widespread acceptance and citation of this particular piece of research, it is interesting to consider the research quality. On what basis were these two claims, the decrease in mobility over time and the poor international standing of Britain, actually made?

Doubts about changes over time

Both of these hugely important claims were made on the basis of evidence from birth cohort studies. The National Child Development Study was set up in 1958 (NCDS1958) with 16,460 neonates and their families, who have been followed up every few years since then for structured interview data about the life, development and influences of each child who is, at time of writing, nearly 50 years of age. Similarly, the Birth Cohort Study was set up in 1970 (BCS1970) with 16,695 neonates. The children of these families are now about 36 years of age. One of the inevitable problems with an ambitious cohort study of this kind is that not all of the original cases continue in every round of interviews. For example, by 1999, of 16,695 children in BCS1970, 2,608 were untraceable, 246 confirmed as emigrated, 109 had died, and 338 refused to take further part (Bynner et al. 2000, p.31). This left 13,394 cases, and this number will decline with every data sweep. The dropout in NCDS1958

is similar. Unfortunately, the cases dropping out at each 'sweep' are not random. Presumably, the sickliest children are more likely to have died, and so on. This selective drop-out introduces a substantial bias for any subsequent analysis, and because it is not random it cannot be compensated for through statistical adjustment. The potential for bias can only be highlighted, and then taken into account by analysts and their users when judging the safety of the results. The figures are also, necessarily, dated, and so any claims about social mobility based on them are not really about Britain as it is now but as it was in previous decades. This is something that David Cameron, the leader of the UK opposition at time of writing, apparently misunderstood when also citing this research for Channel 4 news (<http://www.channel4.com/news/special-reports/special-reports-storypage.jsp?id=4304¶sStartAt=1>).

Blanden et al. (2005) used evidence from both of these cohort studies to make their influential claim that social mobility was decreasing over time in Britain. They examined the adjusted correlations between the income of the children and their parents in each cohort. Thus, strictly speaking, they did not consider social mobility. Their results apply to income mobility – the extent to which the income of the child depends on the income of the parent. This is an important consideration, but not the 'social mobility' that the users of this research talk about (see above). A child who became a university lecturer, born to a father who worked as a fireman and earned as much in real terms as a lecturer, might be an example of social mobility based on education. But this would not show up as *income* mobility. I wonder whether Gordon Brown and others who cite this research are aware of this limitation?

According to Blanden et al. (2005) there is a stronger, partial, correlation between the incomes of parents and children for BCS1970 (+0.28) than for NCDS1958 (+0.17). This is the basis for their claim about declining social (income) mobility. However, two snapshots involving two unrelated groups of children, selected for participation using different techniques by different researchers, are not really the ideal basis for a claim about a trend over time. All we can really say is that the income relationship for the children remaining in the 1970 cohort is stronger than for the children remaining in the 1958 one. Is the adjusted difference between 0.28 and 0.17 large enough for us

to make this claim given that over 3,000 cases would be missing from each analysis through dropout?

Neither of the figures 0.28 or 0.17 is large. We need to square them to estimate the variance in income common to both the parent and child. So, for the 1970 cohort less than 8% (or 28% *of* 28%) of the variation in income of the children is attributable to the parents, and for the 1958 cohort, less than 3% of the variance is attributable to the parents. Or, expressed the other way around, for the 1958 cohort 97% of the variance in children's earnings is unrelated to their parents' earnings, and for the 1970 cohort 92% of the variance is unrelated. Given the inevitable sampling variation in the initial cohorts, the dropout, the differences introduced by having panels of researchers conducting the interviews, and the errors in recalling, measuring and transcribing the figures for income, we may be forgiven for not wishing to base policy on such a small difference. In fact, perhaps the strongest conclusion and the most remarkable observation (not made by the researchers themselves) is how little variation in income is retained across generations of the same family (also see below).

For those readers who still consider the difference between 0.28 and 0.17 to be large enough for successive Secretaries of Education in England to base policy on please consider this next concern. Blanden et al. (2005) did not actually use all of the remaining 13,000+ cases in each cohort for their analyses. Although not made explicit in their summary report, they only considered the incomes for the male children in each family. Thus, their claim should not be about a general decline in social mobility, but a snapshot difference between two cohorts in terms of the income mobility of parents and their sons. Of course, some families did not have sons. These cases are necessarily omitted by Blanden et al. (2005). We might expect, therefore, that each analysis involved at least 6,000+ cases – representing just under half of those 13,000+ cases still involved in each cohort. In fact, Blanden et al. (2005) used only around 2,000 cases from each cohort. Why? What happened to the other 4,000+ cases? For the most part, the next set of omitted cases represents those for whom relevant data was missing. For Blanden et al. (2005) the missing data includes any parents or any sons who had no earned income, even those on benefits. Thus, anyone economically inactive, unemployed, ill, in prison, or who would not reveal their income for any reason was excluded – rather than using their unearned income or

setting their income to zero. Thus, the claim should not be about a general decline in social mobility, but a snapshot difference between two cohorts in terms of the income mobility of parents and sons, both of whom were in declared paid employment. For Blanden et al. (2005) a son with a considerable income himself but born to a long-term unemployed father does not represent any kind of social mobility. I wonder whether Gordon Brown and others realise that this is what the analysis entails?

Blanden et al. (2005) actually used only 2,163 or 13% of the NCDS1958 cases, and 1,976 or 12% of the BCS1970 cases. The reason why is not explained in their paper. No adjustment is made for this 87% to 88% attrition rate in their analysis, and nowhere in their findings do they caution readers about this limitation of their analysis. A different analyst, quite properly making different assumptions about male/female or types and levels of income, could come to a completely different conclusion with a different 12% subset of cases. In fact, Hobcraft (2001) presents the results from eight different combinations of the income measure of parents and the sex of each cohort member. The intergenerational income elasticities range from 0.023 to 0.532 using the same dataset – the NCDS1958. What Hobcraft illustrates (in his Table 6) is how extraordinarily sensitive the results are to the prior analytical assumptions, and this leads him to agree with Breen and Goldthorpe (2001), but for somewhat different reasons, that it is difficult to find any evidence of a change over time in social mobility using the 1958 and 1970 cohorts. Blanden et al. (2002) agree that any results are sensitive to the precise parental income measure used (p.9). Thus, even minor variations in the analytical assumptions made by Blanden et al (2005) for the *different* datasets NCDS1958 and BCS1970 could easily explain a range of results from 0.17 to 0.28 which is much less than the range shown by Hobcraft (2001) for the *same* dataset.

It is interesting to observe in Blanden et al. (2002) that their estimate of intergenerational elasticity for BCS1970 is 0.273. They describe this figure as the result for time averaged parental income – averaged when the cohort member is aged 10 and again aged 16 (see their Table 19). For NCDS1958 they do not time average the result. If they had used the same approach of not time averaging the BCS figures, then their result for either aged 10 or aged 16 would have been the lower figure of 0.210. This would have been much closer to the figure they do quote for NCDS and

which they use to claim a drop in social mobility over time. As a further example of the differences in analytical treatment of the different cohorts, Blanden et al. (2002) explain that, while it would be preferable to have the same income measures for both cohorts in order to make claims about changes over time, this is not possible. They say ‘The NCDS parental income data comes from separate measures of father’s earnings, mother’s earnings and other income... However, the BCS only has data on parents’ combined income’ (p.7). In Blanden et al. (2005), though, this problem is not discussed. We, therefore, need to ask whether their conclusion is safe. Is the size of the difference between the two cohorts large enough to overcome any reservations about bias caused by the non-random whittling away of cases and the differences between the income measures necessarily used in the two calculations?

The authors quote statistical significance as though this can help answer the question. However, the probabilities of statistics are calculated on the basis of a random selection, and so are not appropriate here when 2,000 cases in each cohort have been systematically selected from 16,000 (Gorard 2006a). Quoting standard errors is also inappropriate, since this not a random sample, and quoting standard errors to three decimal places (as the authors do) given that most of the original cohorts are not used in the analysis is rhetorically misleading, by suggesting that the results are accurate to the nearest five parts in ten thousand. The proportion of unexplained missing cases far outweighs the apparent difference between the two cohorts. Given that both cohort studies sampled differently in the first place, asked different questions in different orders, and had the usual levels of measurement error, it would be unwise to base policy on there being a genuine underlying difference. Therefore, the purported explanations of the difference, such as the dangers of closing grammar schools or increasing access to HE, are not needed.

Doubts about international comparators

So, how robust is the second claim, that social mobility is worse than in comparable countries? Using the same kind of data as above, Blanden et al. (2005) claimed that inter-generational income mobility was much less in Britain than in six other comparator states. ‘International comparisons indicate that intergenerational mobility

in Britain is of the same order of magnitude as in the US, but that these countries are substantially less mobile than Canada and the Nordic countries' (p.2). They calculate that the income correlation over generations is 0.27 for Britain, 0.17 for West Germany, 0.15 for Finland, and 0.14 for Canada, Norway, Sweden, and Denmark. On the face of it, this is fairly damning for Britain. They present these figures in Table 2 (p.6) of their paper, which is reproduced here as Table 1. All of the caveats about missing data and dropout discussed so far still apply to these figures of course, and should have the same effect of making readers wary of rushing to easy conclusions based on apparent differences between the scores for each country.

Table 1 - Internationally comparable estimates of intergenerational mobility

Country	Sons born	Sons earning measure	Measure of parental status	Partial correlation
Britain	1970	Age 30	Average parental 1980+1986	.271
US	1954-1970	Age 30	Average parental when son aged 10 and 16	.289
West Germany	1960-1973	2000	Average parental 1984+1988	.171
Canada	1967-1970	1998	Parental when son aged 16	.143
Norway	1958	Average 1992+1999	Father 1974	.139
Denmark	1958-1960	Average 1998+2000	Father 1980	.143
Sweden	1962	Average 1996+1999	Father 1975	.143
Finland	1958-1960	Average 1995+2000	Father 1975	.147

In some countries, the income used by Blanden et al. (2005) for the parents is the father's income (as in the discussion of changes in mobility over time, see above). But for other countries, the average of the income for both mother and father is used where this is available. This reduces the comparability between countries, and it is notable that the inter-generational correlation (final column) seems to be lower on average in countries using father's income (Norway, Denmark, Sweden and Finland) than countries using average parental income (Britain, US, and West Germany). There are also large differences in the ages when the incomes of parents and children were measured in each country. This, again, reduces comparability between the figures. The (higher) correlation for Britain is for sons' income when aged 30, either 14 or 20 years after the parents' income was measured. In Norway, the sons were measured at age 34 or 41, and so were an average of eight years older than their British counterparts, after a gap of 18 to 25 years since the parents' measurement (five years longer than their British counterparts). In Denmark the gap was 18 to 20 years for sons aged 38 to 42 at the time. In Sweden the gap was 21 to 24 years for sons aged 34 to 37, and for Finland the gap between generations was 20 to 25 years for sons aged 35 to 42. These differences in age alone would destroy the claim to differential mobility if, as is possible, the incomes of parents and sons tend to differ more the further apart the readings are taken. The differences in Table 1 might be explained as follows: in general, the greater the gap in years between generation measurements and the older the son, the lower the correlation is. They might have *nothing* to do with the societies in which these figures were collected.

There are other anomalies in Table 1, such as why the figure for Britain is 0.27 not 0.28 as it was when drawing a comparison over time (see above). But perhaps the most important one relates to the year of birth of the children. Why did Blanden et al. (2005) use figures from a 1970 cohort in Britain to compare with a 1958 cohort from Norway when they had a 1958 cohort from Britain as well? Their paper contains no account of why this unlikely comparison is preferred. I asked the authors this question, and their answer was that they needed to use the 1970 figures in order to calculate the average parental income, because they needed this for a fair comparison. This was not possible from the 1958 data. But, the figures they present for Norway are for father's income for 1958, which they had also for Britain and which would

make a much fairer comparison. So this cannot be the reason. Perhaps the answer to my question lies in thinking through what would have happened if Blanden et al. (2005) had used the 1958 figures from their comparison over time. A correlation of +0.17, between the income of those fathers and sons with an earned income, would place Britain with income mobility better than that of West Germany, and much closer to Finland and all of the other Scandinavian countries than to the USA.

The ‘sleight-of-hand’ involved in using the higher 1970 British figures in comparison with the 1958 figures for Norway, when coupled with the concerns over the attrition rate in each cohort, as already discussed, should create considerable doubt over the claims to credibility for this study. This is especially so because the authors do not discuss the problem at all in their paper. The paper contains an internal contradiction and so the conclusion *must* be false, simply in terms of logic. If it makes no difference to these analysts whether we use the 1958 or 1970 British figures for the international comparison, then we cannot also claim a noticeable change from 1958 to 1970. If, on the other hand, they claim that the situation is worse in 1970 than 1958 then we cannot use the 1970 figures to make a fair comparison with the 1958 figures from Norway. It is as simple as that.

In a paper subsequent to my putting these points to the authors, Blanden et al. (2006) still claim to have found a drop in income mobility 1958 to 1970, but they no longer use their own analysis to claim that mobility in Britain is low compared with international comparators. In effect, their conclusions remain the same but they have now removed the data in Table 1 from the view of their readers. This means that readers can no longer see the internal contradiction in their position, or the manifold differences between the datasets for different countries. Instead of relying on their own analysis, the authors make the same point about the poor mobility in Britain by referencing the work of Jantti et al. (2006). Blanden et al. (2006, p.2) say that ‘the level of mobility in the UK is low by international standards (Jantti [sic] et al. 2006)’. I was therefore curious to see the analysis by Jantti et al. (2006), and not unduly surprised to discover that this new research came to conclusions very similar to my own, and actually contradictory to those of Blanden et al. (2006). Jantti et al. (2006) quite properly used the 1958 figures for Britain as the best comparator, and found, as I had predicted, that the partial correlations for the UK are much closer to Finland,

Sweden and Norway than to the US. They conclude - 'The United Kingdom bears a closer resemblance to the Nordic countries than to the United States' (p.5), and this finding is even reflected in the title of their paper - 'American exceptionalism in a new light: a comparison of intergenerational earnings mobility in the Nordic countries, the United Kingdom and the United States'. Both papers are out there for everyone to read and confirm that I am correct here (and I have retained copies of all materials in case they should disappear for any reason). I have no idea why Blanden et al. (2006) claimed instead that the Jantti paper showed mobility in Britain to be low by international standards, because they no longer reply to my academic queries on this matter.

Conclusion

If the foregoing critique is accepted (even in part) it seems that the supposed explanations for poor and declining social mobility in Britain are not needed. However, it is intriguing to note that even in their own terms the explanations suggested do not really fit the timings involved. The 1958 cohort would have sat the 11+ examination for entry to grammar schools around 1970 by which time Anthony Crosland's move towards comprehensivisation was under way, even before the formal abolition of the tri-partite system in 1976. Circular 10/65 from the Department of Education and Science in 1965 had already required all LEAs to prepare for reorganisation of their local schools along comprehensive lines. More importantly, the minority of the 1958 cohort who attended university would mostly have done so at the traditional age around 1977. At that time, the age participation rate for social classes I, II and IIIN was just over 30%, and the age participation rate for social classes IIIM, IV and V was around 6% (Gorard et al. 2007). The 1970 cohort, on the other hand, would have attended university, if they did, around 1989. At this stage, the age participation rate for social classes I, II and IIIN was still only about 36%, and for social classes IIIM, IV and V it was about 9%. These changes over time are too small, and proportionately too much in favour of the less privileged classes, to show up as a subsequent reduction in income mobility. It was in the early to mid-1990s that the number of university places expanded dramatically with the overall participation of both groups combined reaching more than 40%. This was mostly too late for the 1970

cohort, and anyway the growth in HE participation continued to be greater for the less privileged class groups anyway.

Although this is not the point made by the authors, one of the most notable features of the transmission matrices produced by Blanden et al. (2005) from both NCDS1958 and BCS1970 is actually the high level of social mobility in the UK for both cohorts. For example, in both cohorts around 17% of those born to the poorest 25% of families end up in the richest quadrant, and vice versa. If there were no financial inheritance, no inheritance of talent, no nepotism, and *perfect* social mobility then the maximum this figure could be is 25%. The difference from the ideal of perfect mobility in these tables containing 2,000 cases is represented by only about 25 cases in each of the ‘wrong’ extreme cells. Taken at face value, a key policy message could be that Britain has a quite staggering level of social mobility. If we truly value this then we should be wary of doing anything on the basis of poor evidence that might endanger it.

Unfortunately, the unfounded claims by Blanden et al. (2005) that social mobility in Britain is poor and worsening over time have passed into being as social facts. Bad news such as this often seems more exciting for the media and more palatable for commentators and politicians than the truth that change over time is usually less dramatic, less influenced by policy, and more beneficial, than they want to believe (Gorard 2006b). The case study discussed in detail in this paper, chosen because of its profile, is not unique. It illustrates graphically the point that quality and impact are not always related and that, of the two, research quality is more important than impact. High quality research with no impact remains secure as knowledge and might be used in the future. Poor quality research is useless because it leads to insecure knowledge. Poor quality research with high visibility or impact is worse than useless. It could endanger the very thing it was intended to improve.

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